December 19, 1952

Dr. Avram Goldstein
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Dear Dr. Goldstein:

Thank you for sending the E. coli strains on which you have been getting such interesting results. I have been waiting for some preliminary examinations to acknowledge their receipt. You are probably already aware of many of these details, but they may help us to find our bearings.

MSFB is morphologically homogeneous on EMB agar, forming very rough colonies. It grows well on minimal.

Harvard produced two types of colonies: A is smaller, darker, fails to grow at all on minimal. B formerlarger, almost gummy colonies on EMB, and grows reasonably well on minimal (I am just about to test for stimulation by niacin). A and B were present in about equal proportions on the slant as received. After they were separated, they have maintained their distinctive morphology. The differences between A and B are less spectacular than between either of them and MSFB. I would be surprised if a culture as rough as MSFB could produce variants like Harvard A and B, but more characters will have to be examined.

From a plating of abbroth culture of Harvard B, two auxotrophs of about 250 colonies tested were found. Like Harvard A, these both have a complex nutrition which we are trying to run down to get some assessment of the general situation. I have not noticed any instability for characters other than nutritional. It is obviously quite important to verify whether a more or less random variety of auxotroph mutants is produced, or whether they are restricted to one or a few types. I believe you mentioned some indications pointing to the former. I will keep you informed of developments, if any.

I was interested in your remarks on Salmonella transduction. The facts should be distinguished from my speculations. The former, as have been confirmed already by quite a few people are that preparations of bacteriophage can transduce individual traits from one culture to another. The active material has not been separated from the phage despite a variety of kinds of attempt to do so. To make some genetic sense out of this, I have postulated a chromosomal mechanism which can be fragmented during phage growth. The only thing resembling evidence for this is admittedly weak, namely the disorganization of the bacterial nuclei as cytochemically observed in other systems. I am quite eager to hear alternative interpretations from which I may hope to design further experiments.